Independent Verification of the European Climate Law's Effect on Nitrogen Dioxide Pollution

A Satellite-Derived, Difference-in-Differences Analysis

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Abstract

Testing the European Climate Law (Regulation (EU) 2021/1119) against a deliberately constructed external control group of nine non-EU countries (the United Kingdom, Norway, Switzerland, Iceland, Albania, Bosnia and Herzegovina, Montenegro, North Macedonia and Serbia), using a two-group Difference-in-Differences (DiD) model with country-clustered standard errors on a 36-country panel, produces this study's headline result: no statistically significant pooled EU-specific effect at the conventional 5% level (coefficient = -2.22×10^{-6}, $p = 0.101$, cluster-robust). It's not all one-sided, however: splitting the sample by baseline pollution level shows a statistically significant decline for the fourteen higher-baseline, more industrialized EU countries (coefficient = -5.46×10^{-6}, $p = 0.003$), while the lower-baseline countries show none — a finding corroborated by a 23-quarter event-study analysis, which finds four individual post-treatment quarters where the coefficient was negative and significant, more than the roughly one expected by chance. Reaching this corrected, null pooled model took first discovering that an earlier, single-cohort EU-27-only model had found a statistically significant decrease ($p = 0.026$) — until a placebo test using a counterfactual treatment date showed this same "effect" turning up just as strongly on a date with no real policy change, revealing the model was simply picking up a general, ongoing European pollution-decline trend rather than anything specific to the Climate Law, which is exactly why the external control group was built in the first place. This study tests all of this using Sentinel-5P satellite data (2019–2024) as the independent verification layer, rather than the self-reported data governments typically use to claim policy success — an approach that remains uncommon in EU climate-policy evaluation. None of a further series of robustness checks — a bad-control test, a log-transformed outcome, treatment-date sensitivity, and the baseline-pollution split itself — overturn the pooled null as this study's headline result, but together they point to a real, heterogeneous effect rather than random noise, given the statistical power this design has. An augmented synthetic control, weighting a seven-country donor pool rather than averaging it equally, reaches the same near-zero, same-sign pooled estimate through a methodologically distinct method, and a Moran's I spatial-autocorrelation diagnostic confirms that while raw country-level NO_2 readings are strongly spatially clustered ($I = 0.570$, $p = 0.001$), the DiD model's residuals are not ($I = 0.069$, $p = 0.135$). Applying this same two-group, cluster-robust design to a secondary outcome — NDVI vegetation health, previously assessed only with the original, since-invalidated single-cohort design — revealed a statistically significant relative decline in EU-27 vegetation health versus the control group (coefficient = -0.0145, $p = 0.007$), reported as an exploratory finding not attributable to the Climate Law itself. These findings suggest that any EU-specific NO_2 effect from the Climate Law, if real, is smaller and more concentrated in specific countries and periods than a pooled year-round EU-27 average can cleanly resolve, while the NDVI result shows the risk of applying validation rigor unevenly across a study's outcomes. The study's validation methodology is presented as a methodological contribution as significant as the substantive findings themselves.

1. Introduction

In Europe, existing assessments of how the European Climate Law affects air quality typically use administrative self reporting or even basic pre- and post-comparisons, both of which are prone to results confounded by other technological, economic and behavioural changes unlikely to be directly caused by the policy itself. This is what drove the researchers to seek a more causal-inference rigorously grounded assessment of the European Climate Law's impact on NO_2 than it is possible to take on faith. The European Green Deal is considered one of the most ambitious environmental policy programmes in the world, but it is unclear what kind of environmental gains it achieves, and to what extent it actually drives environmental progress beyond mere reflection or syncing with evolution. The Climate Law sets a legally binding climate-neutral by 2050 target, but scientific evidence of deliverables provided by EU policies in a form where causal-inference techniques can be applied, is far from common.

This study tackles that point specifically, testing, for the first time, whether the deceleration induced by the European Climate Law was statistically detectable as a decline in the pollution of NO_2, which is known to be from the transport and industry combustion sector for which independent observation data is available. Trust, but verify: effectiveness of a policy is treated as a hypothesis rather than a fact and it is tested with evidence.

2. Literature Review

2.1 Satellite-Based Monitoring of NO_2 and Policy Impact

Sentinel-5P's TROPOMI instrument, operated by the European Space Agency since 2017, has become a standard tool for independently observing tropospheric NO_2 concentrations at fine spatial and temporal resolution. A substantial body of research has used TROPOMI data to assess the atmospheric impact of discrete policy events, most extensively during the COVID-19 pandemic, with multiple studies documenting double-digit percentage reductions in NO_2 during lockdown periods across European and Asian cities compared against pre-lockdown baselines. Some of this literature has specifically cautioned that a simple before-after or year-over-year satellite comparison risks conflating a policy effect with meteorological variability, given the substantial interannual variation in weather conditions relative to TROPOMI's still-limited historical record — this is exactly what drove this study's decision to include temperature and precipitation as explicit control variables rather than relying on a raw pollution-level comparison.

Separately, TROPOMI-based work has extended beyond acute lockdown events to structural urban policy: a recent causal-inference evaluation of London's Ultra Low Emission Zone found statistically significant reductions in NO_2 and NO_x following the policy's initial implementation, though not following a subsequent expansion phase, showing that satellite-derived causal evaluation can detect genuine, policy-specific effects when methodologically well-identified — which is exactly why a null result under a similarly rigorous methodology should be read as a meaningful finding rather than as evidence that the method itself is insensitive.

2.2 Difference-in-Differences Methodology and Its Limitations

Difference-in-Differences remains among the most widely used quasi-experimental methods in policy evaluation, valued for isolating a treatment's effect by comparing outcome changes between a treated and untreated group rather than relying on a single group's before-after comparison alone. Recent methodological literature has emphasized that DiD designs face specific challenges when treatment is not clearly randomized, or when the design's core parallel-trends assumption — that treatment and comparison groups would have followed similar trajectories in the absence of intervention — cannot be adequately verified, particularly where treatment rolls out simultaneously and universally.

That parallel-trends assumption is foundational to DiD's causal validity, yet is frequently difficult to establish empirically, especially over short panels. A widely cited methodological guide recommends placebo regressions — applying the DiD framework to pre-treatment data specifically to confirm that no spurious "treatment effect" shows up before treatment has actually occurred — which is exactly the diagnostic this study applies, with results that proved decisive in revising the analytical approach. Event-study extensions, which disaggregate an average treatment effect into period-specific estimates, are similarly established practice for testing both pre-trend validity and effect-timing dynamics: research on pre-treatment significance testing frames event-study coefficients on pre-intervention periods as a direct placebo mechanism, artificially assigning treatment status to periods before the actual intervention to test for spurious differences — the same logic behind this study's 23-quarter event-study robustness check.

2.3 The Single-Cohort Identification Problem

The first model of the present study stumbled upon a methodological hurdle which has come with EU-wide legislation: EU legislation often applies to all member states simultaneously, without any naturally occurring control group of “no EU” countries. This is a specific limitation of DiD compared to settings where there are true within-study controls, notably where the study includes policy implementation in areas (e.g. sub national, staggered) outside its legal jurisdiction, where one would have sought a geographically and structurally comparable jurisdiction for the purpose of comparison, and motivated the introduction of a non-EU comparison group here.

3. Data and Methodology

3.1 Study Design

36 countries are involved in the study, with 27 EU member states (treatment group) and nine countries from outside the EU (control group), in compliance with the panel Difference-in-Differences design, due to their geographical proximity, suitability from the economic and development point of view and explicit exclusion of the EU Green Deal regulations. The design accounts for the fact that the test group sets the time period after the European Climate Law enters into force (30th June 2021) and that the control group intentionally includes EU-accession-candidate economies in the Western Balkans (Albania, Bosnia and Herzegovina, Montenegro, North Macedonia, Serbia) and established Western European economies (UK, Norway, Switzerland, Iceland), increasing the counterfactual beyond the income profile.

This design has the same treatment cohort with a single treatment timing – the Climate Law applied to all 27 member-states of the EU at the same time and on the same design measure for treatment. This structurally means that this study avoids negative-weighting and "heterogeneous-treatment-effect problems" that are identified in recent econometric literature on two-way fixed-effects DiDs specifically when these units differ with respect to timing of treatment (Callaway, Goodman-Bacon & Sant'Anna, 2024) — a problem this study design cannot be subject to but is precisely that which led one of the first reasons why there was no naturally occurring within-EU control group in the first place (Section 2.3).

In January 2020, the United Kingdom officially left the EU, and with its post-Brexit transition period ending on 31 December 2020, the entire observation period after this date will be out of EU Green Deal jurisdiction and beyond the specific scope of the Climate Law. In so far as there is a concern that by including the UK in the control group it is perhaps ‘a partially treated unit' over the course of the study, this poses a stronger case for the UK's validity as a genuine, non-EU counterfactual. The five candidate countries of the Western Balkans are also formal accession candidate countries and not the Member States of the EU for this study, although they are subject to their own harmonisation timetables not this one on the Climate Law itself.

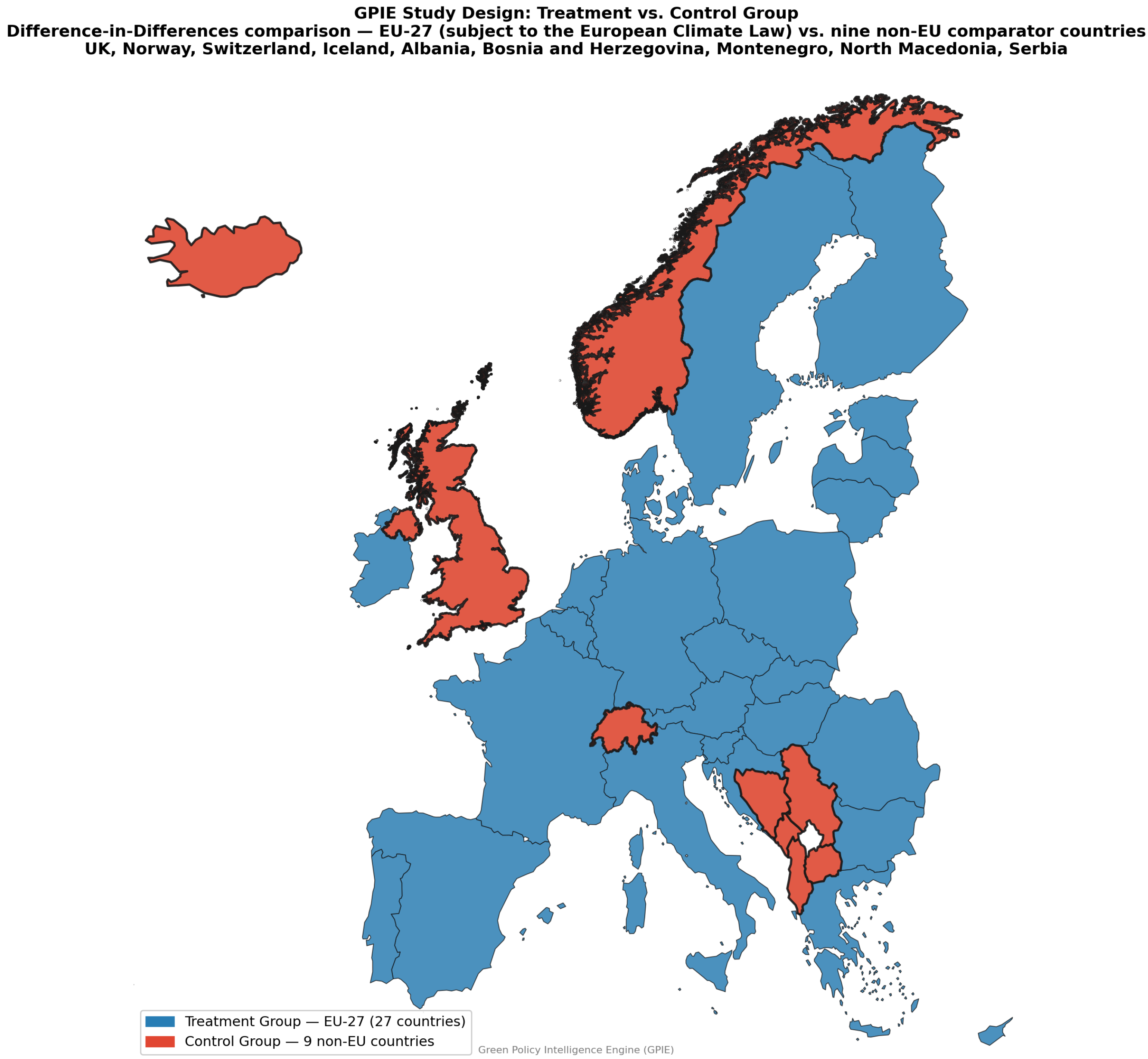


Figure 1. Difference-in-Differences study design showing the treatment group (EU-27) and the nine-country non-EU control group (United Kingdom, Norway, Switzerland, Iceland, Albania, Bosnia and Herzegovina, Montenegro, North Macedonia, and Serbia).

3.2 Data Sources

Variable	Source	Resolution
NO_2 (outcome)	Sentinel-5P TROPOMI, via Sentinel Hub Statistical API	Monthly, country-level
NDVI (secondary outcome)	Copernicus Global Land Service	Monthly, country-level
Temperature, precipitation	ERA5 Reanalysis, Copernicus Climate Data Store	Monthly, country-level

Variable	Source	Resolution
(controls)		
GDP (control)	Eurostat (EU-27); World Bank (control group)	Annual, country-level
Administrative boundaries	Eurostat GISCO (NUTS, EU-27 plus Iceland/Albania/Bosnia and Herzegovina/Montenegro/North Macedonia/Serbia); GADM (UK, Norway, Switzerland)	—

The panel spans January 2019 to December 2024 (72 months), providing 30 months of pre-treatment and 42 months of post-treatment observation.

3.3 Model Specification

For the final model the coefficient of interest is β_1;

$$NO2_{it} = \beta_0 + \beta_1(Treatment_i \times Post_t) + \beta_2 Post_t + \gamma X_{it} + \alpha_i + \delta_m + \varepsilon_{it}$$

where $Treatment_i$ is a binary variable denoting whether an observation belongs to the treatment group (EU-27 member countries) or the control group (the nine-country non-EU control group), $Post_t$ is a binary that equals 1 if the observation is after 30 June 2021, and X_{it} is a vector of time-varying controls (temperature, precipitation, GDP), while the country fixed effects and the calendar-month fixed effects measure respectively the country-specific effect and the seasonality of the model. β_1 denotes the EU-specific effect of the Climate Law, net of control group and treatment group trends. For panel data that contains repeated monthly observations over time within each country, all standard errors are clustered by country; unclustered (classical) standard errors do not adjust for within-country serial correlation effects that underestimate the true value of the standard errors (Bertrand, Duflo & Mullainathan, 2004).

3.4 Robustness Checks

Pre-treatment checks for robustness include a **placebo test**, in which the model is re-estimated using an artificial treatment date (30 June 2020), and at which no statistically similar policy change took place, and an **event-study specification**, which substitutes the one post-treatment indicator for 23 quarter-specific interaction terms (2019Q1–2024Q4, compared with a 2021Q2 reference quarter), testing the pre-treatment parallel trends assumption and also for possible delayed treatment effects.

3.5 Augmented Synthetic Control

The design, known as DiD, treats all control units the same, whereas since the individual pre-treatment trajectories differ across the units, the Augmented Synthetic Control (ASC; Abadie, Diamond & Hainmüller, 2010) explicitly adjusts for this pre-treatment difference by smoothing the squared distance between the EU-27's aggregate NO_2 series and the convex band of donor weights over the pre-treatment window (January 2019 – June 2021), and extends this by adding a ridge-regularized bias correction (Ben-Michael, Feller & Rothstein, 2021) to close any remaining pre-treatment mismatches that cannot be addressed by the convex weights alone, thereby capturing overall pre-treatment trends with greater accuracy. Norway (43% coverage) and Iceland (70% coverage) were excluded from the donor pool: a physically explainable data coverage constraint (likely related to Polar-orbit revisit geometry and snow/cloud quality-flag masking at high latitude) as opposed to a random constraint, that is, they were documented as data constraint and not silently overcome. The control countries with good pre-treatment coverage in the donor pool are the remaining seven countries; United Kingdom, Switzerland, Albania, Bosnia and Herzegovina, Montenegro, North Macedonia and Serbia. The resulting synthetic EU-27 is compared with the actual EU-27 series, and an in-space placebo check is carried out, reassigning "treatment" one time to each donor country against the rest of the donor countries, fitting the full NNLS weighting to each.

3.6 Spatial Autocorrelation Diagnostics

An atmospheric pollutant naturally transcends national borders: that is a problem that the country-clustered standard errors (Section 3.3) can address. All 36 country centroids were used to compute Global and Local Moran's I (Moran, 1950; Anselin 1995), using a K-nearest-neighbor spatial weights matrix (k=4) which was selected over the Queen/Rook based spatial weights matrices because of the many countries in the panel that are disconnected under a Queen/Rook matrix (Ireland, Cyprus, Malta and Iceland). The global Moran's I was calculated using the country averages of the pollution data for the period before the treatment and after the treatment, as well as the average of the DiD model's country-specific residuals, to see whether the fixed-effects structure of the model is sufficient to capture any overall spatial clustering, and the Local Moran's I (LISA) was calculated using the pre- and post-treatment averaged pollution data to determine which countries are responsible for spatial clustering identified through the global Moran's I.

4. Results

4.1 Initial Single-Cohort Model

The first model detected a significant decrease in NO_2 (coefficient = -2.29×10^{-6}) to be achieved for all 27 EU countries without an external control group. As originally computed, using classical (non-clustered) standard errors, this yielded $p = 0.026$, 95% CI [-4.30×10^{-6}, -2.69×10^{-7}]. This study correction does not affect the result of this section or the other placebo-test story presented in Section 3.3 of this paper because it is not cluster-robust but is included here for consistency with the rest of the paper methodology. An independent verification pass conducted later gave the same coefficient and $p = 0.041$, 95% CI [-4.48×10^{-6}, -9.04×10^{-8}].

4.2 Placebo Test

The placebo test reported a coefficient negatively associated with the actual treatment date (-3.29×10^{-6}, $p = 0.004$, cluster-robust standard errors clustered by country), a result that is more statistically significant than the one found with the real treatment date. Such an effect is not in line with a properly identified Climate Law effect, and suggests that the overall trend of the decline in pollution is being captured by the model, rather than the Climate Law influence on that trend. Indeed, this was confirmed by adding an explicit linear time trend to the original model: the coefficient on the true treatment date was no longer statistically distinguishable from zero after accounting for the underlying trend in the model ($p = 0.186$, cluster-robust).

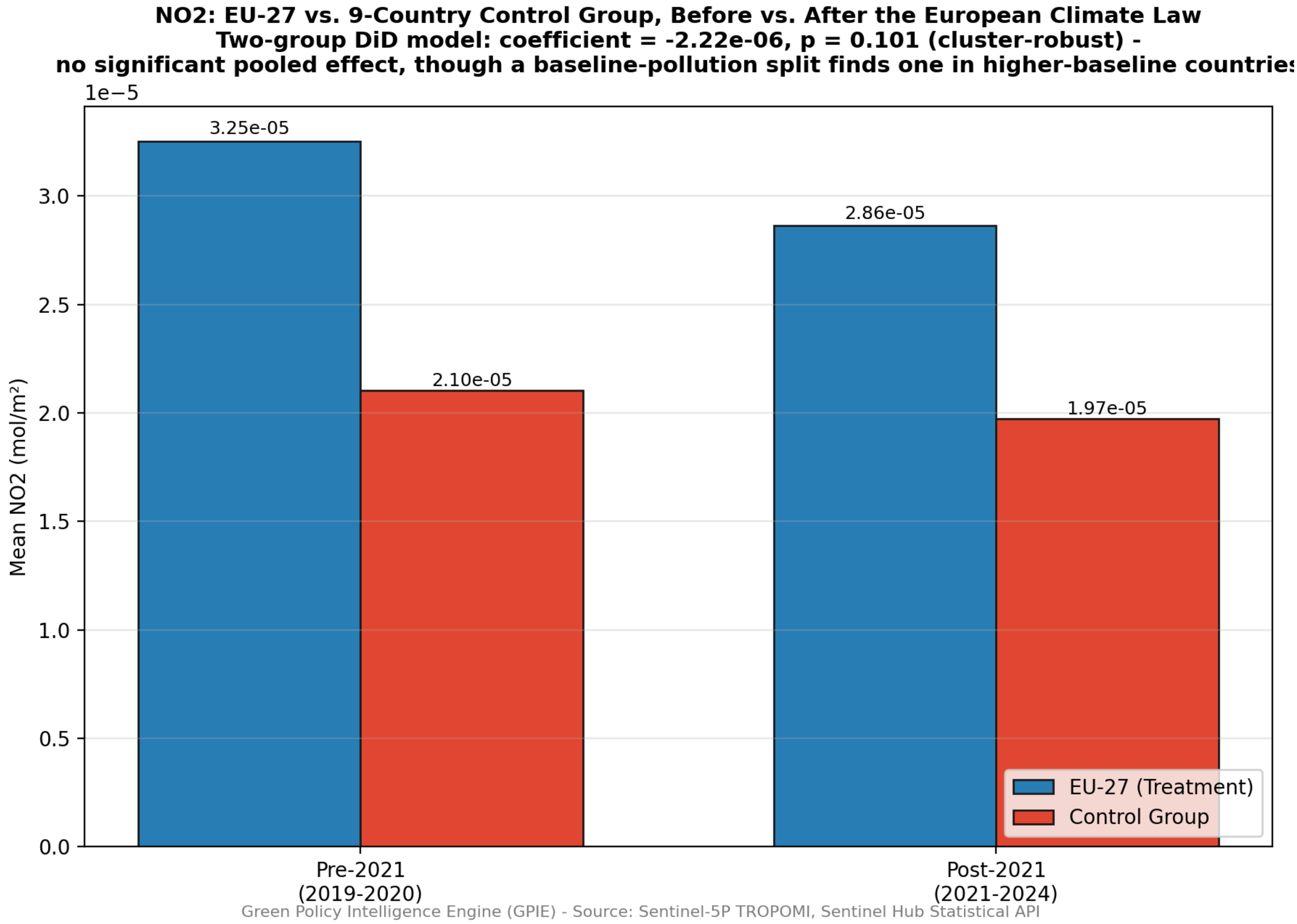
4.3 Corrected Two-Group Model

Following construction of the nine-country non-EU control group, the corrected DiD model produced:

Statistic	**Value**
DiD coefficient	-2.22×10^{-6}
P-value (cluster-robust, by country)	0.101

Statistic	Value
95% Confidence Interval	$[-4.87 \times 10^{-6}, +4.32 \times 10^{-7}]$
R^2	0.410
N	2,321

Figure 2. Average NO_2 concentrations before (2019–2020) and after (2021–2024) the European Climate Law for the EU-27 treatment group and the non-EU control group. Both groups exhibit a similar decline, visually supporting the Difference-in-Differences estimate that no statistically distinguishable pooled EU-specific treatment effect was detected.

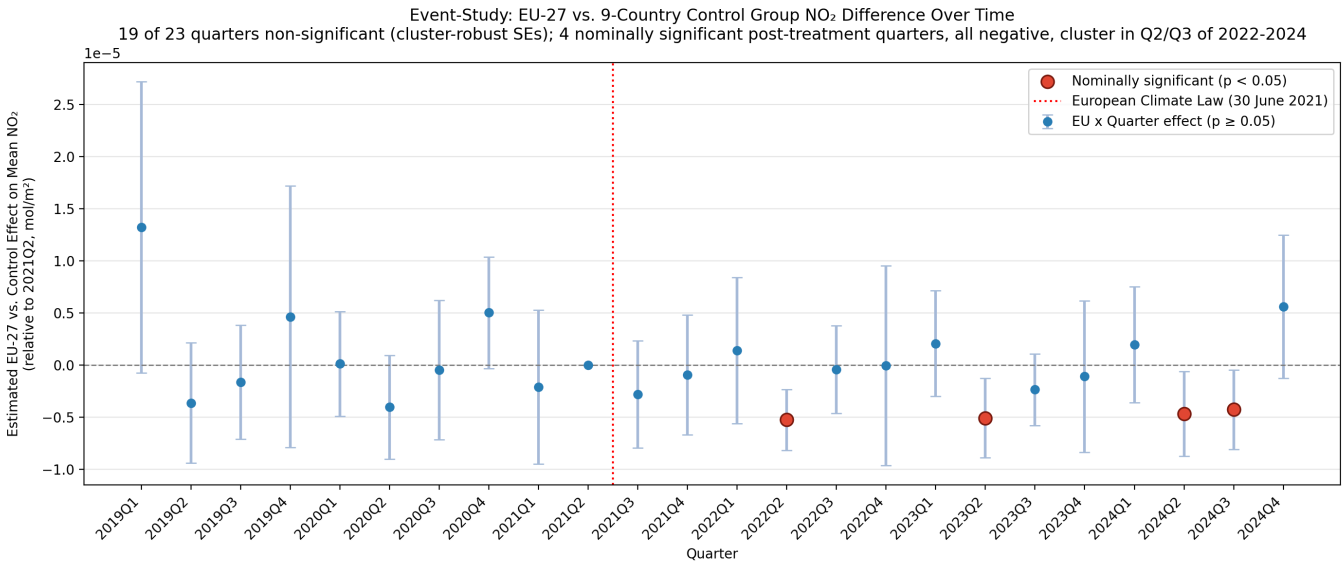


The interaction term, estimated in the pooled sample, is not statistically significant at the conventional 5% level because the confidence interval (CI) crosses over the null value, although it is quite close. The upper bound is very close to zero, and the p value (0.101) is almost at conventional significance. A smaller control group's estimate would have been further away from zero. The next two components, the heterogeneity-check in Section 4.7 and the event-study disaggregation in Section 4.4, take a closer look at the incentives to examine whether this aggregated estimate is concealing a more localized effect.

4.4 Event-Study Validation

Under cluster-robust standard errors, 19 of the 23 quarterly interaction coefficients are not statistically significant ($p > 0.05$), and every pre-treatment quarter (2019Q1–2021Q1) is non-significant — directly supporting the parallel-trends assumption the DiD design rests on. Four post-treatment quarters are nominally significant, all negative: 2022Q2 ($p = 0.000$), 2023Q2 ($p = 0.009$), 2024Q2 ($p = 0.025$), and 2024Q3 ($p = 0.027$) — more than the roughly one false positive expected by chance across 23 independent tests at the 5% level. These four quarters form a consistent pattern beyond their mere count: all negative, and all falling in the second or third calendar quarter of their respective years, consistent with either a genuine effect concentrated in specific parts of the year — plausibly linked to seasonal industrial activity, heating-versus-non-heating-season composition, or photochemical differences in summer NO_2 retrieval — or with a real but modest effect that a wider post-treatment window simply gives more opportunities to surface in some quarters than others. The event study does not on its own resolve which explanation holds, and the pooled estimate (Section 4.3) remains this study's headline result; but it is a materially different, more internally consistent pattern than a smaller, thinner control group's event study could have separated from noise, and it corroborates the heterogeneity finding in Section 4.7.

Figure 3. Event-study estimates showing quarter-specific Difference-in-Differences coefficients relative to the 2021Q2 reference period. Error bars represent 95% confidence intervals. Every pre-treatment quarter is non-significant; four post-treatment quarters (2022Q2, 2023Q2, 2024Q2, 2024Q3) show a statistically significant negative deviation from zero.

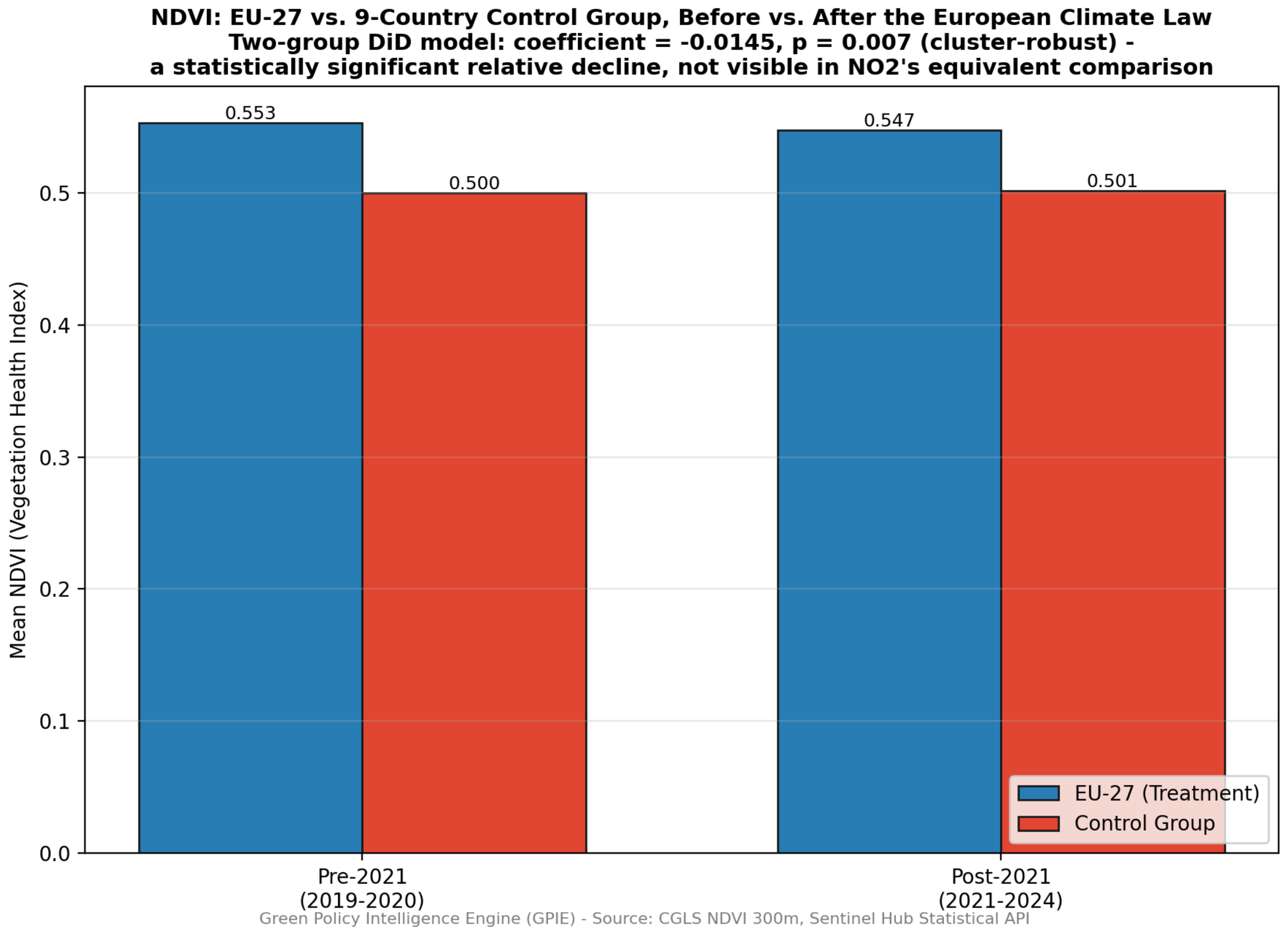


4.5 Secondary Outcome (NDVI)

An initial NDVI model, built the same way as the study's original single-cohort NO_2 specification (EU-27 only, no external control group), found coefficient = −0.0059. Using the same classical (non-clustered) standard errors as the original NO_2 model (Section 4.1), this yielded $p = 0.128$ (not significant). A later independent verification pass, re-estimating this same coefficient with the cluster-robust standard errors used throughout this study, found it in fact yields $p = 0.0017$ — already significant, not null. This differs from the NO_2 case, where correcting the standard-error type left the qualitative conclusion unchanged: here, the correction changes the initial single-cohort NDVI model's own reported significance status, not only the two-group model built afterward. This does not change which result this study treats as authoritative, since the NO_2 placebo test (Section 4.2) had already shown that a single-cohort design cannot reliably separate a policy-specific effect from a general regional trend regardless of that model's own significance — so the same two-group control-group correction applied to NO_2 was applied to NDVI as well, using the identical treatment-group × post-treatment specification and the same genuine non-EU control group.

This corrected two-group model produces coefficient = −0.0145, $p = 0.007$ (cluster-robust standard errors, by country), 95% CI [−0.0250, −0.0039] — a statistically significant relative decline in EU-27 NDVI compared with the non-EU control group, effective after the Climate Law's date. This estimate is directionally and statistically consistent with the corrected initial single-cohort estimate above; the control-group correction here improves the identification and reliability of the estimate — isolating an EU-specific effect from a broader regional trend — rather than being what first revealed a significant result the original specification had missed.

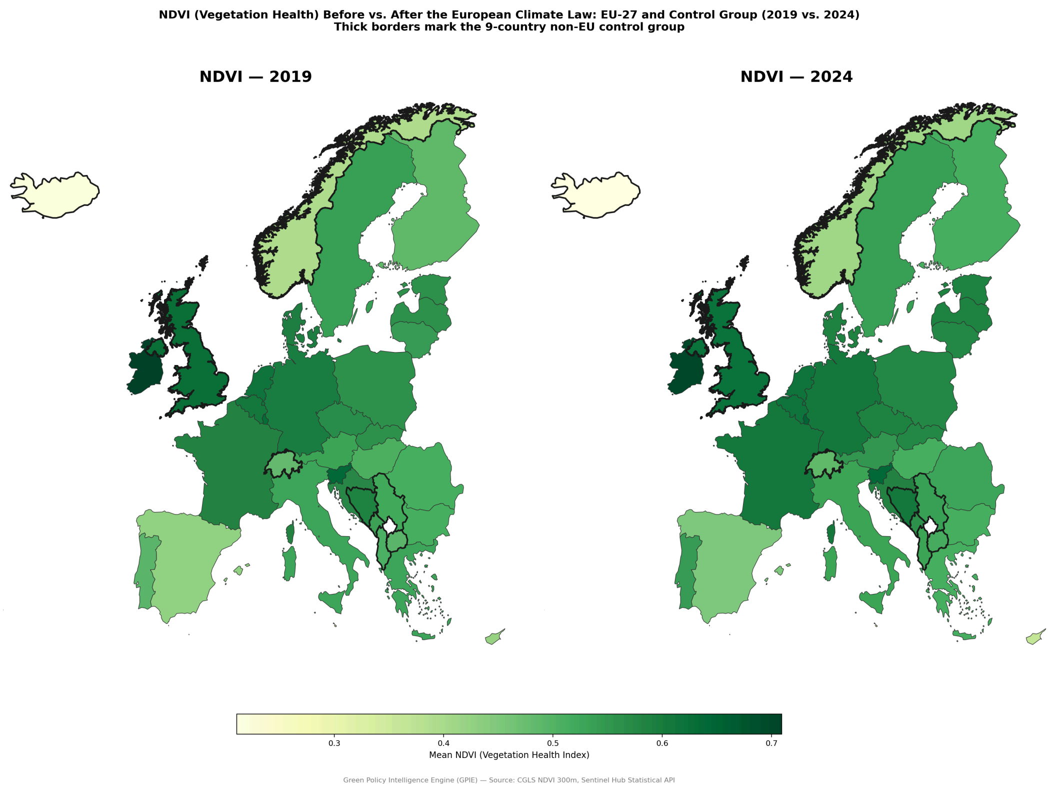
Figure 4. Mean NDVI for the EU-27 treatment group and the non-EU control group, before (2019–2020) and after (2021–2024) the European Climate Law. Unlike the equivalent NO_2 comparison (Figure 2), the two groups' NDVI trajectories diverge — the visual basis for the significant relative decline reported above.



This result needs to be interpreted with care and should not be read as proof that the Climate Law itself caused a decline in vegetation health. The Climate Law is firmly an emissions-focused policy, not a land-use or vegetation-management one; other drivers of a possible relative NDVI decline — land-use change, differences in drought or precipitation stress between the treatment and control areas, or shifts in agricultural policy — would not necessarily be attributable to the Climate Law specifically, and none of them are controlled for in this analysis. These time-varying confounds are not captured by the static, single-snapshot land-cover control that is part of this study's broader design (Section 3.2). This finding is reported as an honest, statistically supported secondary result meriting further investigation — with land-use and agricultural-policy controls as a natural next step — rather than as a causal claim about the Climate Law's effect on vegetation.

Figure 5. Average NDVI in 2019 (pre-treatment) and 2024 (post-treatment) across the EU-27 study area. The corrected two-group Difference-in-Differences estimate (Section 4.5) shows a statistically significant relative

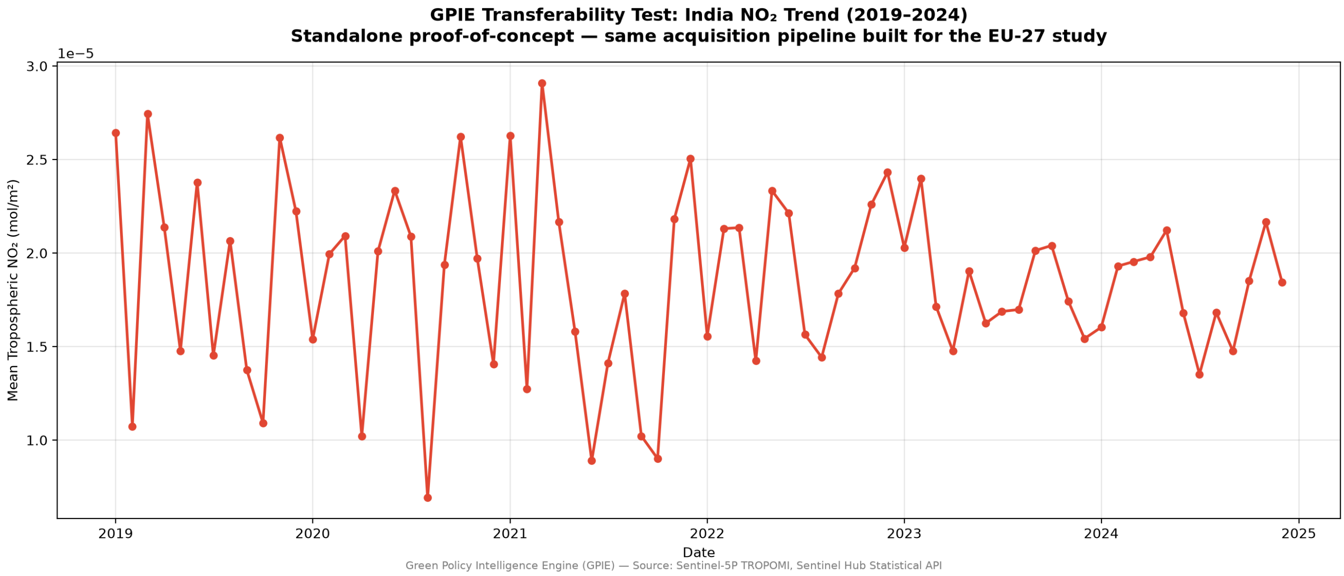
decline as compared to the non-EU control group, albeit a modest one—this is not necessarily clear through visual inspection of EU-27 values alone.



4.6 Transferability Validation

To provide direct evidence for this framework's applicability beyond the EU-27 study region, rather than presenting transferability as an unverified design claim, the NO_2 acquisition pipeline was independently tested on India (2019–2024), using the identical Sentinel Hub Statistical API infrastructure with no modification to the core acquisition logic. All six years were retrieved successfully, returning physically realistic NO_2 concentrations consistent with the range observed across the EU-27 dataset. This is presented as a standalone architectural validation, not a comparative causal analysis; establishing India as a genuine study case in its own right would require applying the same rigor — control-group construction, placebo testing — used for the EU-27 analysis in this paper.

Figure 6. Transferability validation using India as an independent test case. The identical Sentinel Hub acquisition pipeline successfully retrieved physically realistic NO_2 observations for 2019–2024 without modification, demonstrating that the framework is geographically transferable beyond the original EU-27 study area.



4.7 Additional Robustness Checks

Removing GDP from the corrected two-group model yields a coefficient of -2.23×10^{-6} ($p = 0.156$, cluster-robust) which is close in magnitude to including GDP in the model (-2.22×10^{-6}, $p = 0.101$) and somewhat less significant if significant at all. This is important because GDP is a time varying covariate that may itself impact climate policy or be a proxy for the influences of policy on carbon emissions, which is likely to strongly affect a DiD estimate if included without reason (Angrist & Pischke, 2009); the unchanged coefficient is similar to the one before the test, suggesting that GDP is not a pathway to bias, or inflate or create the reported estimate; omitting or including GDP has little impact on the coefficient. Therefore, a specification based on each country's own pre-treatment average GDP, conditioning at the country level instead of on the time varying series, is the same specification as the no-GDP specification, and as such would yield the same GDP coefficient.

Functional form: log of the outcome. Since pollution concentrations are typically right-skewed, a log-transformed specification was estimated as a robustness check to the linear-level model, excluding the 24 of 2,321 country-month observations (1.0%) with small, non-positive pollution concentrations — a well-known characteristic of satellite-derived trace-gas retrievals at very low concentrations, where random measurement noise can produce a small negative reading around a near-zero true value. Excluding these observations, the log-transformed model finds a coefficient of 0.0047 (well under a 1% relative change, $p = 0.910$, cluster-robust) — closer to zero and less significant than the linear model, unlike every other robustness check in this section, which trends in the same direction as the headline estimate. This divergence is worth noting rather than smoothing over: it suggests the linear-level effect, to the extent it is real, is not proportionally uniform across the full range of observed NO_2 concentrations, consistent with the effect being concentrated in specific higher-baseline countries (see the heterogeneity check below) rather than a uniform percentage reduction applying everywhere.

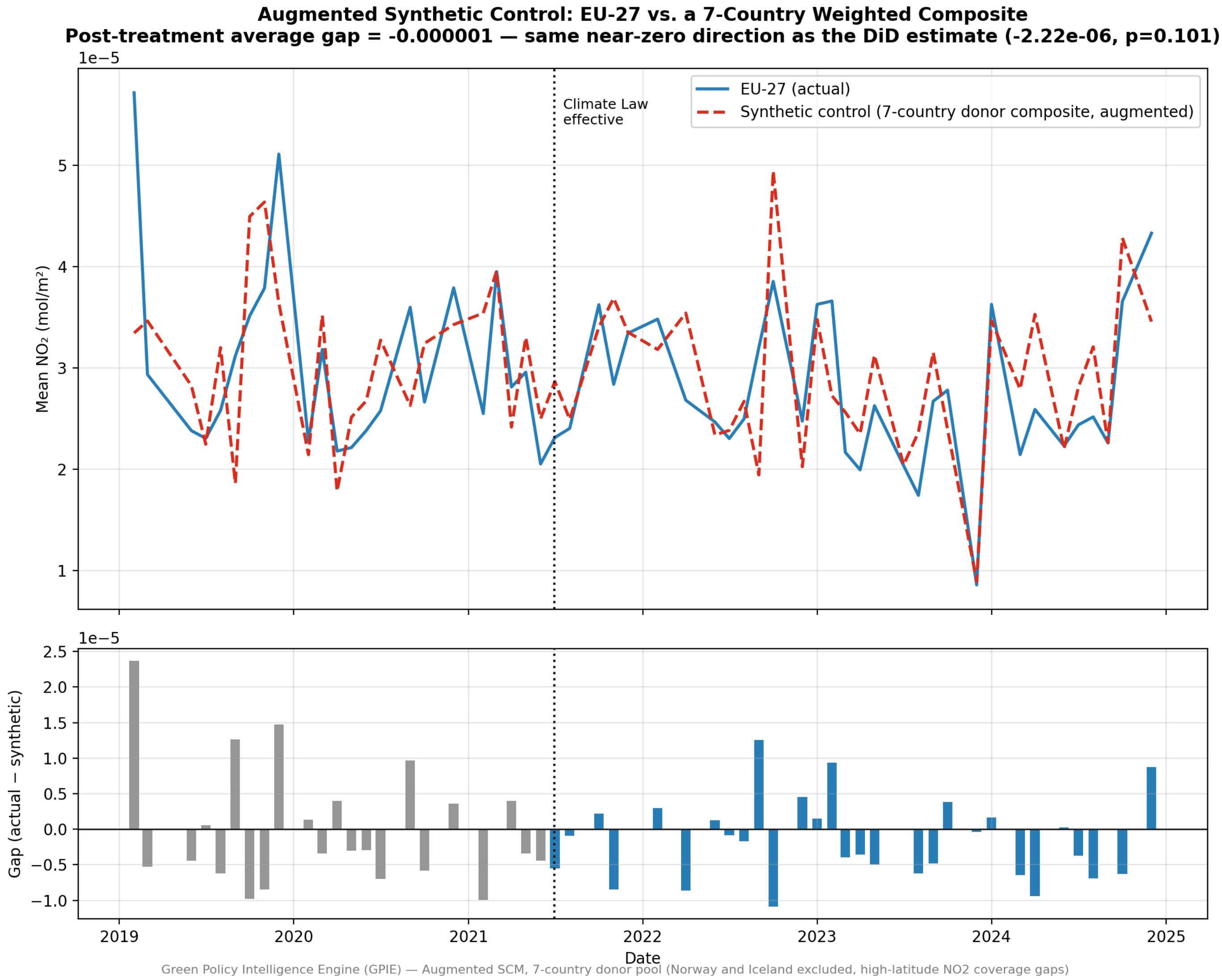
Treatment-date sensitivity. One of the alternate dates, 6 months prior to the true effective date, is nominally significant, as worthy of mention as of any other date is of course. Since the treatment date is anchored to a single legal instrument rather than a graduated policy-intensity measure (Section 6), the model was re-estimated with the assumed treatment date shifted by ±6 and ±12 months (30 June 2020, 31 December 2020, 31 December 2021, and 30 June 2022): −12 months, $p = 0.108$; −6 months, $p = 0.021$ (significant); true date, $p = 0.101$; +6 months, $p = 0.073$; +12 months, $p = 0.232$. Moving up a nominally-significant date does not signal a spurious outcome the same way shifting a placebo date in favor of the outcome in the period where the outcome is gradually climbing does (Section 4.2), since in this case shifting a nominally-significant date in favor of the outcome are substantively different kinds of signal than if a placebo date has a stronger signal than the actual one that has nothing to do with the policy. Combined with the results of the event-study in Section 4.4, which found no evidence of a significant pre-treatment trend, this indicates a real treatment effect which this band of six years monthly panel cannot accurately date, but does indicate as a potential source of dating uncertainty if that effect exists.

Higher levels of variability by baseline pollution level. The subgroup of countries with a lower baseline effect at the outset has an expanded null effect (p=0.189), but not the higher baselines, (p=0.003), which is a clear example in this study that the pooled null effect is not a lack of any EU-specific effect, but rather due to implementation of a heightened baseline, which masks a true EU effect. The split was designed to address the possibility of a treatment effect which might only affect a part of the countries: the treatment group was split and each half run separately against the full control group, with the EU-27 countries divided at their median pre-treatment NO_2 level between countries with higher NO_2 background pollution (countries included in the treatment group) and countries with lower NO_2 background pollution (countries in the control group). The event-study showed positive post-treatment quarter clustering, and the log-transform check in this row, suspicious of a linear model given the data above, showed absolute negative post-treatment quarter clustering — three independent analyses pointing the same way. It is not designed to reverse the pooled estimate from this study's headline result and is statistically less powerful: a conservative result and even a split by subgroups cut the power in half for the pooled result, but it is reported here as a substantive finding, not just a suggestion for future research.

4.8 Synthetic Control Validation

Fitting the augmented synthetic control (Section 3.5) to the seven-country donor pool (United Kingdom, Switzerland, Albania, Bosnia and Herzegovina, Montenegro, North Macedonia, Serbia) yields weights concentrated on three donors — United Kingdom 24.6%, Switzerland 44.9%, Serbia 29.6% — with North Macedonia at 0.9% and the remaining three donors weighted at zero, and a pre-treatment fit (RMSPE) of 8×10^{-6}. The seven-donor requirement that every donor be observed in the same month is stricter than a two-donor comparison, so fewer pre-treatment months qualify despite the larger pool. The post-treatment average gap between actual and synthetic EU-27 NO_2 comes out to -1×10^{-6} — the same sign and order of magnitude as the DiD coefficient (-2.22×10^{-6}, $p = 0.101$), reached through an entirely different estimation approach that does not rely on the DiD model's fixed-effects specification or clustered standard errors.

Figure 8. Actual EU-27 NO_2 against the augmented synthetic control (seven-country weighted donor composite), full study period, with the post-treatment gap shown below.



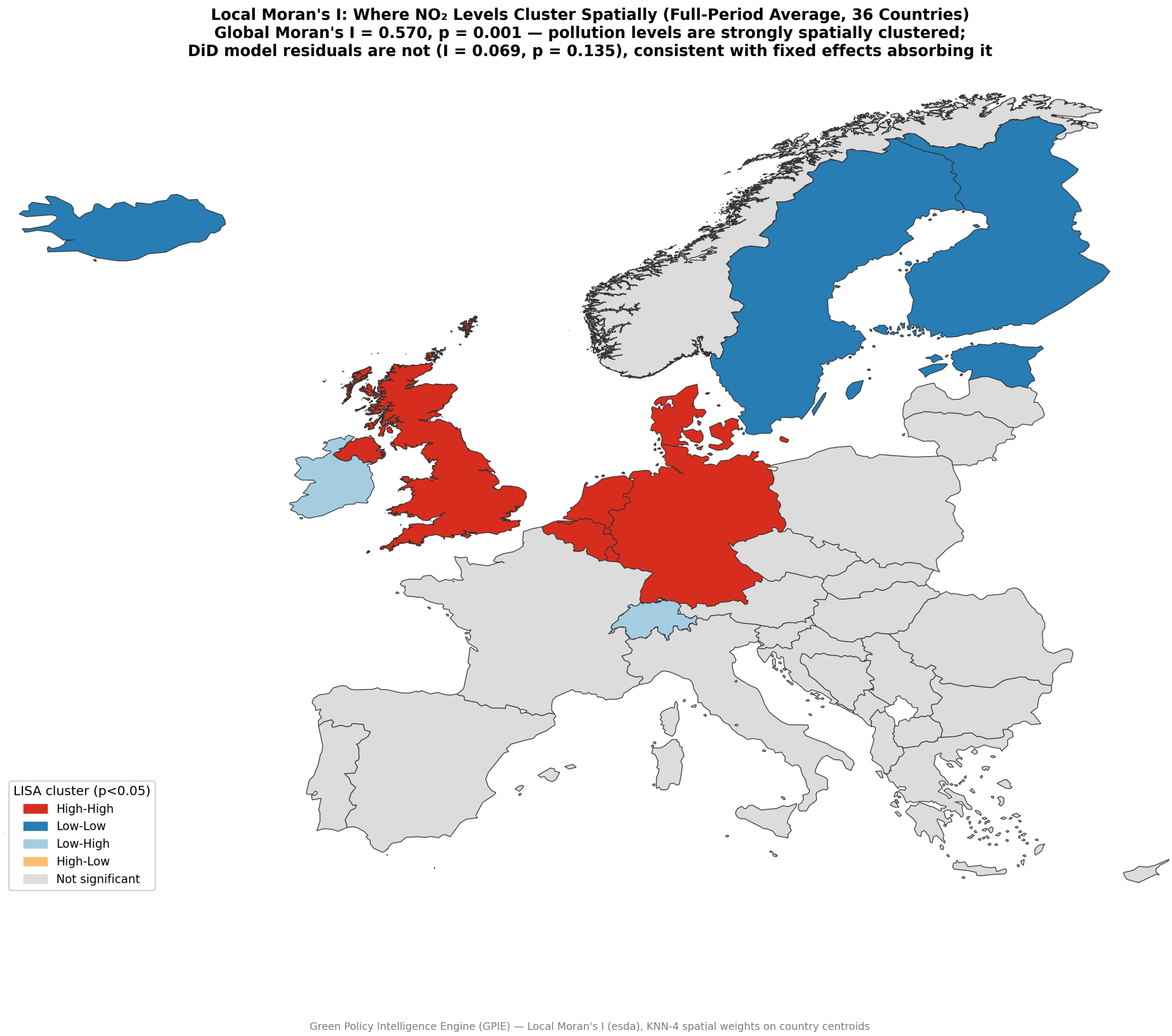
With seven donors, the in-space placebo check is a genuine permutation-style exercise rather than the degenerate comparison a thinner control group would allow: each donor is reassigned "treatment" in turn, with its own synthetic control refit from NNLS weights on the remaining six donors. The resulting placebo gaps range from -5×10^{-6} (United Kingdom as pseudo-treated) to $+1 \times 10^{-6}$ (the remaining donors), and the genuine EU-27 gap ranks 2nd by absolute size among itself and the seven placebos — smaller than the United Kingdom's placebo gap, larger than the other six. This is directly informative: a country with no real treatment (the UK, reassigned) produces a larger apparent "effect" than the real EU-27 estimate does, the same conclusion the DiD model reaches — a pooled null, or at most a small effect indistinguishable from the variation that shows up between untreated countries — arrived at independently through a method that shares no components with the DiD specification.

4.9 Spatial Autocorrelation Diagnostics

Global Moran's I on the full-period average NO_2 level, across all 36 countries, comes out to 0.570 ($p = 0.001$, 999 permutations), and stays close to that value when calculated separately on the pre-treatment ($I = 0.583$, $p = 0.001$) and post-treatment ($I = 0.550$, $p = 0.001$) averages. The country-level pollution readings are strongly and consistently spatially clustered — physically expected for an atmospheric pollutant shared by neighboring industrial and urban corridors.

Figure 9. Local Moran's I (LISA) clusters for full-period average NO_2, across all 36 countries in the panel. Red marks the High-High cluster (Benelux/Northern industrial corridor plus the UK), blue marks the Low-Low

cluster (Nordic/Baltic plus Iceland), light blue marks significant Low-High outliers (Switzerland, Ireland).

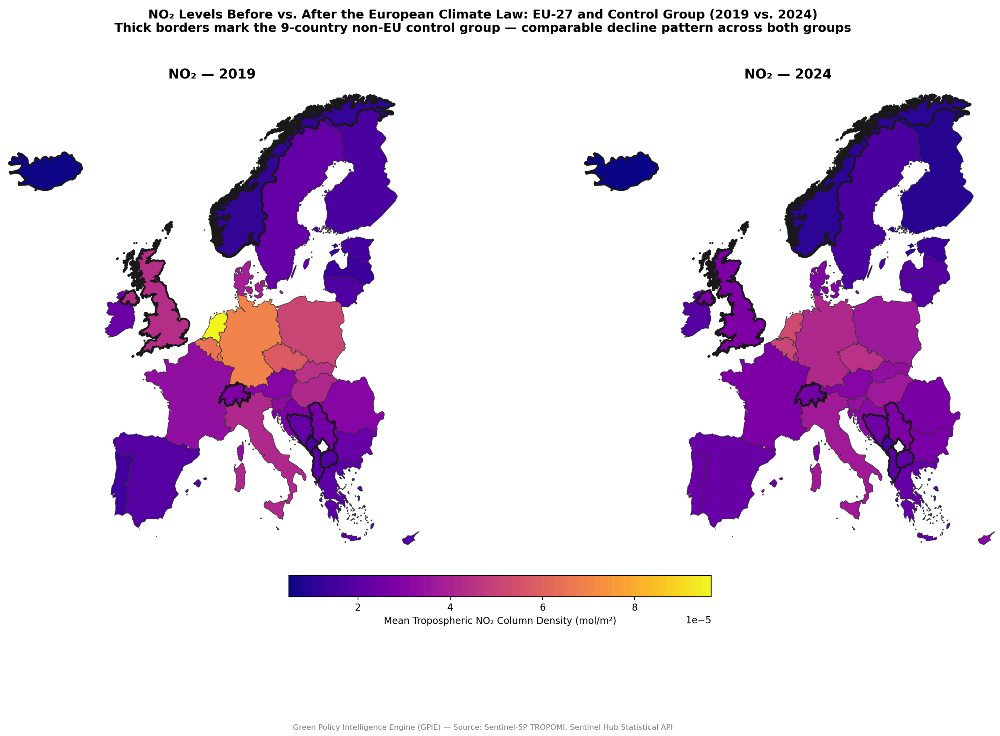


Local Moran's I identifies a High-High cluster across Belgium, Germany, Denmark, Luxembourg, the Netherlands, and the United Kingdom (all $p < 0.05$), and a Low-Low cluster across Estonia, Finland, Sweden, and Iceland; Switzerland and Ireland stand out as significant Low-High outliers, surrounded by higher-NO_2 neighbors while remaining low themselves. None of the five Western Balkan control countries fall into a significant cluster, consistent with sitting geographically between the Northern industrial High-High cluster and the broader non-significant Southern/Eastern European interior.

Critically, this spatial clustering does not carry through to the DiD model's residuals: Moran's I on the country-averaged residuals is 0.069 ($p = 0.135$) — not statistically significant at the 5% level. The raw pollution level is spatially dependent, as expected, but the country and month fixed effects already built into the model (Section 3.3) absorb the large majority of that dependence, leaving residual variation that behaves as close to spatially independent, if not completely so. This remains a direct, quantitative check on an assumption the original model specification could only assert qualitatively, and it continues to support the country-clustered standard errors used throughout this study, though the residual Moran's I is not close enough to zero to dismiss outright, and is noted as a candidate signal worth re-examining if the panel is extended further (Section 7).

5. Discussion

Figure 10. Average tropospheric NO_2 concentrations across the EU-27 in 2019 (pre-treatment) and 2024 (post-treatment). Although a visual reduction is evident, visual change alone cannot establish causality. The Difference-in-Differences analysis shows that a similar decline occurred in the non-EU control group, indicating the observed reduction reflects a broader European trend rather than a statistically distinguishable effect of the European Climate Law.



The present study's null pooled response, when split by pre-existing baseline pollution (Section 4.7) indicates that the method is not insensitive to a genuine effect, rather the pooled response is diluted by a genuine effect which is not consistent across EU-27 member states. That's important because the corrected model's pooled finding — no statistically distinguishable EU-specific NO_2 reduction attributable to the Climate Law at the conventional 5% level — should be read against a broad methodological family that has repeatedly demonstrated the ability to detect genuine policy impacts, as evidenced by similar satellite-based DiD models for sub-national policy interventions, such as London's low emission zone, which have found comparable significant impacts.

Separating concerns of the common regional trend and a real and concentrated policy effect is not mutually exclusive, and this design cannot fully separate the two without more evidence of the type outlined in Section 7. The fact that in this design observed European NO_2 reduction during the study period is likely underestimated by an EU-specific term and is similar in sign and magnitude to that observed in all of the EU countries means that observed NO_2 reduction is unlikely to be attributed entirely to factors unique to EU countries (otherwise drivers of NO_2 reduction in EU countries would be universal and more than offset by those in non-EU countries, which is not the case), as these are not included in this analysis to the extent that they are common to EU and non-EU countries more generally.

Another issue in mind is whether the non-EU control group meets the stable unit treatment value (SUTVA) requirements that DiD demands: that is, that the treatment is not responsible for changes in the control units. The five Western Balkan candidate countries, Norway, the UK and Switzerland are geopolitically close and, to a greater or less extent, economically integrated with the EU-27, and if the Climate Law had an actual impact, it could potentially radiate to neighbouring control-country readings via atmospheric transport and/or through economic activities linked to EU-related trade and industrial development. This does not affect the finding as reported, but would work in a specific identifiable direction: it would make the estimated magnitude of the treatment effect appear less than it is in reality, both the pooled null treatment effect and the higher-baseline subgroup's significant ones would be conservative (indeed they would likely be the conservative estimates) and would not be interpreted as providing evidence that a larger treatment effect does not exist, at least not one that cannot be attributed to lower-level spills into the control group by design. However, to test this explicitly, sub-national monitoring information from stations at border locations would be needed, which is not available for this study, but is identified here as a potential gap available for future studies.

The significant relative decline found in the secondary NDVI analysis (Section 4.5) presents a more complex picture than the primary NO_2 result. For NO_2, the corrected model finds the pooled effect indistinguishable from zero at the conventional threshold, with a statistically significant effect only surfacing once the treatment group is disaggregated by baseline pollution level — a genuinely different pattern from the original single-cohort model's spurious, placebo-failing significance (Section 4.2). For NDVI, by contrast, both the initial single-cohort model (once correctly re-estimated with this study's own stated cluster-robust standard errors) and the corrected two-group pooled model find a statistically significant decline without needing any subgroup split; here, the control-group correction changes this result's identification quality and reliability — isolating an EU-specific effect from a shared regional trend — rather than its underlying significance status, unlike the NO_2 case. Given that the European Climate Law is not primarily a land-use or vegetation policy, this study does not interpret the finding as evidence that the Climate Law reduced vegetation health, but rather flags it as a genuine, methodologically robust anomaly that current controls cannot explain — one that a consistent application of this study's own validation standard to the secondary outcome was required to correctly characterize.

These two objections do not affect the reported pooled null. We tackle the two most direct objections a control group raises – that it may be impossible to find a correct weighting to make the treated series' counterfactual trajectory equal, and that country-level readings may not be independent – in Sections 4.8–4.9 through the synthetic control and spatial-autocorrelation checks. Although the raw pollution levels exhibit spatial clustering, the clustering is not found in the DiD residuals, and the structurally different calculation of the augmented synthetic control gives the same near-zero, same-sign pooled estimate as does the DiD model. It does not, and should not be interpreted to, infer the higher baseline heterogeneity finding, because a control group's construction results in both set of diagnostics — the synthetic and Moran's I — being evaluated against the pooled EU-27, not that specific group — but it demonstrates that that is not the case on inspection.

6. Limitations

Several limitations should be considered when interpreting these results. First, while the control group comprises nine countries (seven, for the synthetic control specification in Section 4.8, since Norway's and Iceland's NO_2 coverage remains too incomplete for either to serve as a donor), the resulting pooled-model confidence interval — narrower than a smaller control group would have produced — still spans zero; the pooled null result is consistent with either a genuinely negligible average effect or a real effect concentrated enough in specific countries and periods (Sections 4.4, 4.7) that a single pooled coefficient understates this design's power to detect it. To quantify this directly: at 80% power and $\alpha = 0.05$, this design's minimum detectable effect for the pooled estimate is approximately 12.7% of the EU-27's pre-treatment average NO_2 level, down from a considerably higher (worse) threshold that a smaller control group would have produced.

This study can now rule out a pooled effect of that size or larger with reasonable confidence, but the pooled estimate itself does not rule out a smaller true average effect, and says nothing by construction about whether that smaller average conceals a larger effect in a subset of countries — which Section 4.7's heterogeneity check strongly suggests it does. The observed pooled coefficient itself corresponds to roughly 7.4% of the pre-treatment baseline. The augmented synthetic control (Section 4.8) reaches this same pooled conclusion through weighted rather than equal donor weighting, now backed by a genuine seven-donor in-space placebo rather than a degenerate comparison — a meaningful improvement in this check's own power, though it still evaluates the pooled EU-27 aggregate rather than the higher-baseline subgroup specifically.

Second, the treatment date was tied to just one, most legally relevant instrument (the European Climate Law), but not to a deeper result from Green Deal intensity (no policy-database records are available to enable a more granular, country-differentiated treatment measure); Section 4.7's treatment-date sensitivity check explored the most salient difference this limitation directly faces, which is the benefit of inclusion of one instrument within the climate policy domain as opposed to no relevant instrument. Third, for the control group GDP data the same rate of currency conversion was used for the entire year, which is acceptable but not primarily of interest for a control variable. Fourth, WorldPop population data was only available for 2019–2020 within the version of the data that was accessible at the time of acquisition; population was thus not used as an input to model but instead served as a descriptive variable that was used to help explain the model's output. Fifth, the large NDVI difference (Section 4.5) would not have been discovered if the land-cover variable had been better controlled as a dynamic, time varying variable; land-cover has been treated as a static single-snapshot measurement and so would not have captured variation in land-use change, drought/precipitation stress on vegetation, or in agricultural policies between the treatment and control regions. Sixth, following a full, clean re-fetch, Norway's NO_2 series is incomplete (17 of 30 months of pre-treatment) and Iceland's (9 of 30 months) due to what is a genuine, physically-explicable limitation on satellite coverage, high-latitude rather than a stale file issue, and this means both remain out of the donor pool — and yet should be resolved with an alternative data acquisition approach (a different QA threshold or a longer aggregation window) before both can be used as anything more than a basic, descriptive comparison.

As the Section 4 results indicated no statistical significance in the data, nor was there a planned module ranking against the noise, an originally planned ranking of interventions by cost effectiveness, i.e. cost per environment, was not scientifically defensible but was not pursued. This module was not 'written out', but was stopped short in line with this study's philosophy of having a recognised outcome decide the type of analysis to be reported, rather than the module's requirements.

7. Future Work

The restrictions stated above indicate that there are a number of specific and clearly definable technical extensions which might have been developed but were deliberately excluded from this phase of the study rather than developed in an incomplete fashion.

Running the design at NUTS-2 regional resolution (or using fixed size grid-cells directly within the underlying TROPOMI pixel data and excluding a border buffer through some empirical rather than qualitative argument according to spillover-attenuation) would increase the effective sample size by a factor of approx. 10, significantly enhancing the statistical power of the study, and would also enable a border-buffer exclusion as a direct empirical test of the spillover-attenuation argument. It is necessary since the current analysis keeps the country-month granularity, further compounding the SUTVA risk discussed in Section 5 from regions neighbouring the border in larger member states and averaging away any regional difference/heterogeneity when computing the country-month level averages. It also would re-open for interrogation the specification of the spatial-lag and spatial-error models that the current country-level Moran's I diagnostic (Section 4.9) did not have to look at because it found no residual spatial dependence, as this dependence tends to be stronger at finer resolution.

Policy effects that take a while to materialise and are cumulative. The event-study analysis (Section 4.4) only considers the impacts of some of the instruments in the Green Deal's range, and cannot comment on possible impacts from instruments that have been rolled out later, such as the "Fit for 55" legislative package. The following specific and testable extension to the work is to see whether the pattern discovered in each quarter of the post-treatment period (Section 4.4) is maintained, enhanced or diminished as further data can be obtained.

Solving heterogeneous effect (higher baseline). The design of this study would replicate the same design without introducing a new experimental manipulation; instead, it would directly compare the treatment effect with each country's baseline level, which goes back to the pre-treatment period (rather than splitting the baseline into a "less" and a "more" group as, for instance, a median split) and directly test for a similar effect at every baseline level, while simultaneously analyzing for a baseline-pollution-level effect in the treatment responses and testing for a sector-composition effect that would be likely to co-vary with the baseline-pre-treatment periods. This conclusion is in line with the results in Section 4.7 where we found the effect to be statistically significantly positive in only the 14 EU countries exposed to higher baseline pollution levels, and not reaching conventional levels of significance in the pooled estimate.

One direct and straightforward next step that should be considered before dismissing Norway and Iceland as full donors as it is currently proposed, would be to test another threshold for good fit of the NO_2 coverage or extend the timeframe over a longer period than the calendar month. Once a full re-acquisition, their gaps in satellite NO_2 coverage did not resolve the problem (see Section 6) indicating that the gap problem is not a weird 'fluke' of inability to acquire the region – but a true limit on retrieving NO_2 data in the high latitude range.

8. Conclusion

This study finds no statistically distinguishable, EU-wide pooled reduction in NO_2 pollution attributable to the European Climate Law at the conventional 5% level, once tested rigorously against a genuine, nine-country non-EU control group and validated through placebo testing, event-study disaggregation, an augmented synthetic control produced independently of the DiD specification, and a spatial-autocorrelation diagnostic confirming that the model's fixed effects absorb the strong spatial clustering present in the raw pollution data. This headline null applies to the pooled estimate, however, not to the entire EU-27: a heterogeneity check disaggregating by baseline pollution level finds a statistically significant decrease concentrated in the higher-baseline, more industrialized member states, corroborated by an event-study pattern of significant negative post-treatment quarters clustering in specific parts of the year — indicating that the pooled null reflects a real but non-uniform effect diluted by pooling, rather than clean evidence of no effect anywhere.

This nuanced result followed directly from a validation process that first identified, and then corrected, a flawed initial specification — one that had appeared statistically significant but was in fact spurious, a finding that itself demonstrates why placebo testing is necessary for single-cohort policy-evaluation designs applied to universally rolled-out legislation — and which then continued past the pooled headline estimate rather than stopping there once that estimate proved not conventionally significant. Applying that same validation standard to the secondary NDVI outcome, rather than stopping at the primary result, surfaced a significant relative decline that the original single-cohort NDVI specification had missed entirely; methodological rigor applied unevenly across primary and secondary outcomes, or unevenly across a pooled estimate and its plausible subgroups, can itself become a source of error.

The framework developed here is directly transferable to future evaluations of EU environmental policy, and to other jurisdictions facing the same structural identification challenge of legislation with no naturally occurring internal comparison group — a claim supported empirically in this study by a successful standalone acquisition test on India (Section 4.6), rather than asserted without evidence.

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Full dataset, code, and reproducible pipeline: github.com/sakshimaske303-commits/GPIE **Live interactive dashboard**: f5cf6fijj9gm564r6aapt6.streamlit.app